PRICE COMPETITION AND CONCENTRATION IN SEARCH AND NEGOTIATION MARKETS: EVIDENCE FROM MORTGAGE LENDING

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ABSTRACT. This paper examines the impact of bank consolidation on mortgage rates in order to evaluate the extent to which mortgage markets are competitive. Mortgage markets are decentralized and so rates are determined through a search and negotiation process. The primary effect of a merger therefore is to reduce the number of partners available with whom to negotiate, although it can also change the characteristics of the product, and impact the search effort of consumers. Using a Canadian merger as a case study, we find that, overall, consolidation had little effect on rates suggesting that, on average, the mortgage market is fairly competitive. However, a decomposition of the aggregate treatment effect reveals important heterogeneity in the impact of the merger. We find that consumers gathering multiple quotes are affected by the merger, while those who do not search are not. These results suggest that market power originates in large part from the presence of asymmetric search costs.

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1. Introduction

Although North American and European mortgage markets are highly concentrated, pricing may still be competitive. Understanding the extent to which pricing in mortgage markets is competitive is central to the design and evaluation of policies meant to regulate mortgage markets. These include antitrust policies regarding the approval of bank mergers, regulations constraining the scope of bank activities, and policies affecting banks' costs of funding such as those targeting capital or securitization.

In theory, several institutional features suggest that mortgage markets should be competitive: contracts are homogeneous, rates are negotiable, and, due to loan securitization, for a given consumer costs are mostly common across lenders. Moreover, in the market that we study, lenders are fully protected against the risk of default by a government insurance program, which standardizes the lending conditions across financial institutions. These features allow informed consumers to gather multiple quotes, and obtain an interest rate that reflects the expected lending cost, even with a small number of competing lenders. In practice, however, consumers differ in their ability to understand the subtleties of financial contracts, and their willingness to negotiate and search for multiple quotes. Indeed, recent surveys in Canada and in the United States show that, while some buyers get multiple quotes when shopping for their mortgage contract, nearly half only get one. This heterogeneity can induce price dispersion, even among homogeneous contracts, in which lenders offer better rates to consumers that are able to gather multiples quotes and/or that have good bargaining skills.[1]

In this paper, we take advantage of the quasi-experimental variation created by a merger between two important lenders in Canada to evaluate the level of price competition among mortgage providers.[2] Unlike retail markets often used to study the effect of concentration on prices, mortgage markets are decentralized, and contract terms for each individual are determined through a search and negotiation process. Therefore a lender's ability to exercise market power vis-a-vis a borrower depends not just on differentiation like in standard retail markets, but also on the borrower's search and negotiation ability. In this context a merger effectively reduces the number of negotiating partners accessible to consumers. We are interested in studying the impact of losing a lending option on consumer-level transaction rates. For borrowers with poor search and negotiation ability, there should be little or no impact of losing a lender. In contrast, searchers may be affected depending on how competitive the market is. If the market is not perfectly competitive, losing a lender will lower borrowers' bargaining power, and lead to higher prices. On the other hand, if the market is competitive, the number of lenders in consumers' choice-sets should not affect the discount they receive.

[1]The presence of financial intermediaries, such as mortgage brokers and real-estate agents, can attenuate some of these information frictions, but also create new problems such as exclusion (some banks refuse to deal with brokers), and repeated interactions leading to tacit collusion between brokers and banks. Hall and Woodward (2010), for example, find that mortgage brokers in the United States capitalized on their information advantage to over-charge borrowers leading up to the housing crisis.

[2]For confidentiality reasons we cannot reveal the exact details of the merger.

To conduct our empirical analysis we exploit a detailed data-set containing a random sample of all new mortgages administered by the Canada Mortgage and Housing Corporation; a federal crown corporation responsible for insuring the loans of home buyers who require insurance under the National Housing Act. These data provide administrative information on contract terms, household characteristics, and market-level characteristics. The richness of this consumer information, in combination with lender-level location data, allow us to identify the causal effect of the loss of a lending option on transaction interest rates by exploiting the sharp changes in the choice-set of consumers induced by the merger.

We use a difference-in-difference estimation strategy to quantify this effect. This strategy relies on the fact that most consumers shop for their mortgage contract locally, and that the merger differentially impacted the structure of local-markets across the country. In particular, roughly sixty per cent of consumers in our data-set had both merged entities in their neighborhoods prior to the merger, while the remaining had only one or neither. The latter set of borrowers represents a natural control group. This strategy has been recently used to study the impact of mergers in gasoline markets (e.g. Hastings (2004), Hastings and Gilbert (2005), and Houde (2011)), in the cement industry (e.g. Hortacsu and Syverson (2007)) and in the health-care industry (e.g. Dafny et al. (2011)), among others. See Ashenfelter et al. (2009) for a survey of this literature.

The main contribution of this paper is to document the important heterogeneity in the reaction of firms and consumers to a merger. This is in contrast to most of the literature studying the effects of horizontal mergers, which has focused on posted prices. We exploit observed differences in the choice set of consumers and their financial characteristics to estimate heterogenous treatment effects. We also estimate the distribution of treatment effects across unobserved consumer types using a semi-parametric estimator proposed by Athey and Imbens (2006).

Our results can be summarized as follows. We estimate that the merger led to an average interest rate increase in treated markets of 6 basis points (bps). For a $100,000 loan, we estimate that the merger led to a $4 increase in monthly payments (or $240 over the five years of the contract). Overall this effect is small, and suggests that the average consumer is able to extract a large share of the transaction surplus through search and negotiation. However, decomposing the effect of the merger by observed transaction characteristics reveals important asymmetries. Consumers transacting with the merged entity experienced the largest price increase: up to 15 bps, compared to 5 bps for consumers selecting a competing institution. These results are consistent with the idea that the merger improved the quality of the complementary services offered by the merged entity, and that it changed the mix of consumers that each lender serves. We also find that the merger led to more intense search efforts: the aggregate probabilities of dealing with a broker or switching financial-institutions (a proxy for search intensity and effort) increased by nearly 6% in the treated markets (relative to comparable controls). This likely attenuated the market power increase associated with the merger.

Using the estimator proposed by Athey and Imbens (2006) we analyze the distribution of the treatment effect across unobserved borrower types. Since we condition on a rich set of characteristics, the different percentiles of the distribution can be interpreted as the unobserved search and

negotiation ability of consumers. Low quantiles are borrowers who received a relatively big discount (e.g. searchers), while high quantiles are borrowers paying high rates (e.g. non-searchers). We find that losing a lender option raised interest rates between 7 and 9 bps for consumers in the lower and middle percentiles of the conditional transaction rate distribution, but is statistically indistinguishable from zero in the top 35%. This result confirms that only consumers gathering multiple quotes are adversely affected by the merger. In contrast, consumers who are unable or unwilling to negotiate pay a price that is strictly a function of their willingness to pay and/or the common posted interest rate, which is not directly affected by the loss of a lending option. This leads us to conclude that market power in mortgage markets originates in large part from the presence of heterogeneous search costs and negotiation abilities.

These results have important policy implications. On the one hand, our analysis suggests that, from the point of view of consumers who search, the market is fairly competitive. Although rates increase, the extra interest cost for the average searcher caused by the merger is relative small, especially considering the fact that our analysis focuses exclusively on local markets with a small number of lenders (i.e. between five and eight). On the other hand, we also find that the rates paid by borrowers who do not search are not affected by the degree of competition between lenders: access to fewer lender options does not lead to higher rates for these borrowers. Therefore, policies designed to increase competition, or at least to prevent increases in concentration, such as restrictions on merger activity or bank bailouts, may not be effective for those consumers unwilling to search or unable to negotiate. Instead, policies aimed at improving financial literacy, or reducing the ability of lenders to price discriminate between consumers based on negotiation ability are more likely to be effective at reducing interest rates for these consumers. Given the large amount of residual price dispersion that we document in this market, it is likely that these policies would be effective at increasing consumer welfare.

We are connected to two important literatures. First, there is an extensive literature analyzing bank mergers, particularly in the U.S. This stems largely from the increased number of mergers following the Riegle Neal Act of 1994. Berger et al. (1999) provide a detailed discussion. Despite this attention, a lack of consumer-level data has made it difficult to analyze the effect on transaction prices, and therefore most studies have focused on the impact of mergers on deposit services (fees and rates). For instance Prager and Hannan (1998) find that bank mergers in the U.S. led to a decrease in deposit rates. Focarelli and Panetta (2003) use Italian deposit data and find a similar rate decrease in the short-run, but determine that in the long-run the gains in efficiency due to the merger resulted in rate increases. Closest to our approach, Sapienza (2002) uses Italian data and finds that interest rates on business loans increased or decreased after a merger depending on market shares. She finds that mergers leading to high levels of concentration were associated with the highest rate hikes. Consistent with our results, she also provides evidence that borrowers with either many or very few outside options are relatively unaffected by changes in market power, whereas borrowers with average outside options are the most affected. Panetta, Schivardi, and Shum (2009) use the same data as Sapienza and find that mergers lead to a better correspondence

between interest rates and risk. That is, risky borrowers pay a higher rate post merger while less risky borrowers pay less.

Second, this paper is related to the literature on information frictions in financial markets. Most of this literature has focused on risk and problems of adverse selection (see for instance Adams, Einav, and Levin (2009)). Less attention has been paid to information frictions stemming from search costs as in the mortgage market. Hortaçsu and Syverson (2004) study the role that search frictions play in generating the large amount of dispersion in the fees that investors pay in the mutual fund industry.

The paper is structured as follows. Section 2 describes the Canadian mortgage markets, focusing on market structure, mortgage insurance and contract type, pricing, negotiation, and shopping habits. Section 3 presents the data. Section 4 introduces a simple bargaining model to illustrate the theoretical effects of mergers on negotiated prices. Section 5 discusses our identification strategy, and Section 6 presents the results. Section 7 concludes. We relegate some tables to the Appendix.

2. The Canadian mortgage market

2.1. **Market structure and mergers.** The Canadian mortgage market is currently dominated by six national banks (Bank of Montreal, Bank of Nova Scotia, Banque Nationale, Canadian Imperial Bank of Commerce, Royal Bank Financial Group, and TD Bank Financial Group), a regional cooperative network (Desjardins in Quebec), and a provincially owned deposit-taking institution (Alberta's ATB Financial). Collectively, they control 90 per cent of assets in the banking industry and are called the "Big 8."

The market was not always this concentrated. Until the early 1990s the Canadian residential-mortgage market also featured a large number of trust companies. Trusts make mortgage loans, funding them by issuing guaranteed investment certificates and accepting deposits. At the time the main difference between trusts and banks was that trusts were more lightly regulated with regards to reserve requirements. In particular, trusts did not have to hold reserves against mortgages, while chartered banks did. This provided trusts with a competitive advantage in the mortgage market due to lower cost of funding. Cross-ownership between the two types of institutions was not permitted until the 1992 revisions to the Bank Act. Following these revisions banks and trusts were granted almost identical powers, making them undifferentiated products from the point of view of consumers.[3]

As a result of the Bank Act revisions and a series of bad residential and commercial loans that created solvency and liquidity issues for the trusts in the 1980s, Canadian chartered banks acquired the majority of trust companies over the course of the following decade. The merger wave led to the six largest banks controlling approximately 80 per cent of the mortgage market – almost double their 1980s market share. These mergers all resulted in significant expansion of the merged

[3]There were still differences in ownership structure (trust companies could be closely held - and commercial ownership of trusts became common, while banks had to be widely held to prevent ownership concentration) as well as in supervisory authority (banks are federally regulated whereas trust companies can be federally or provincially regulated), but these differences are unlikely to affect consumer demand. In 1992 trusts were given full consumer lending powers, and banks were permitted to offer in-house wealth management advice (fiduciary services).

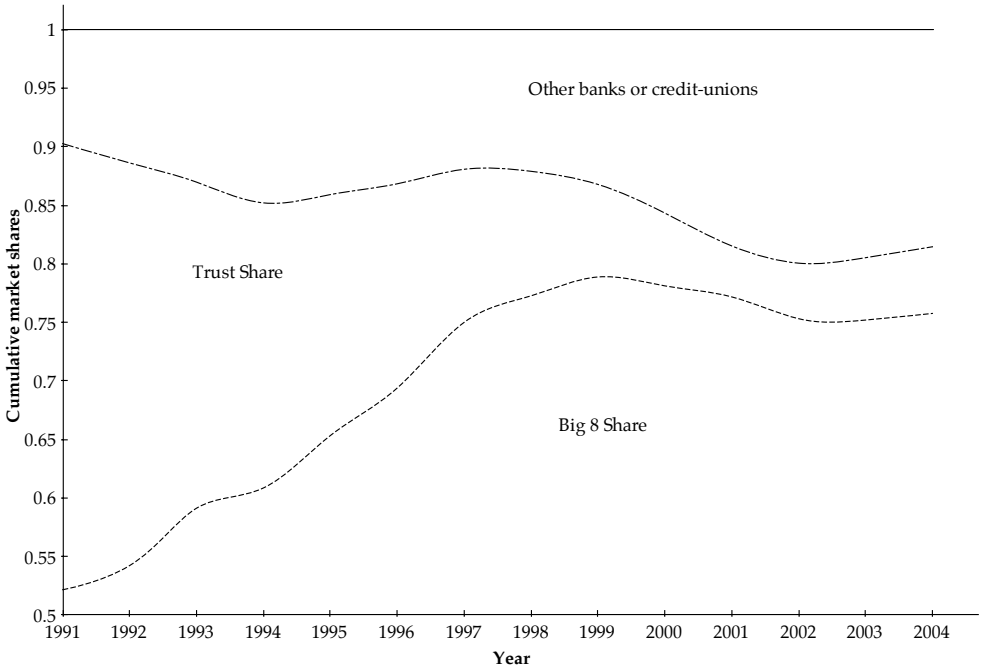


FIGURE 1. Evolution of the market share of financial institutions among new insured mortgage contracts (smoothed)

entity's branch network since in each case the Canadian Competition Bureau required little or no forced divestiture of branches. Figure 1 presents the evolution of the mortgage-market share of the main lending groups – The Big 8, Trusts, Credit Unions and other banks. Today, there are still many trusts operating in Canada, but they are small and their influence on the mortgage market is much less than it was prior to 2000.

The major trust acquisitions were: Canada Trust & Toronto-Dominion (2000), National Trust & Scotia Bank (1997), Montreal Trust & Scotia Bank (1994), Royal Trust & Royal Bank (1993), and Central Guaranty Trust & Toronto-Dominion (1993). A more detailed discussion of the major Canadian bank mergers is presented in the Appendix. Our empirical analysis focuses on one of these major acquisitions.

2.2. **Mortgage contracts and mortgage insurance.** There are two types of mortgage contracts in Canada – conventional mortgages which are uninsured since they have a low loan-to-value ratio, and high loan-to-value mortgages, which require insurance (for the lifetime of the mortgage). Today, approximately 80% of newly issued mortgages fall in the latter category. The primary insurer is the Canada Mortgage and Housing Corporation (CMHC), a crown corporation with an explicit backstop from the federal government.[4] Our analysis focuses on insured mortgages.

[4]There are a number of private insurers as well, the only one in existence during our sample was Genworth Financial, which also has an explicit government of Canada guarantee, albeit for 90 per cent. CMHC's market share during our sample averages around 80 per cent.

All insurers use the same strict guidelines for insuring mortgages.[5] First, borrowers with less than 25% equity must purchase insurance.[6] Second, borrowers with monthly gross debt payments that are more than 32% of gross income or a total debt service ratio of 40% will almost certainly be rejected.[7] CMHC charges the lenders an insurance premium, ranging from 1.75 to 3.75 per cent of the value of the loan – lenders pass this premium onto borrowers. Insurance qualifications (and premiums) are common across lenders and based on the posted rate. Borrowers qualifying at one bank, therefore, know that they can qualify at other institutions, given that the lender is protected in case of default.

During our sample period, nearly all mortgage contracts were fixed rate, among which over 85 per cent had a 5 year term (the second most common term is 36 months). A 5 year fixed-rate mortgage contract must be renegotiated every five years, which in effect acts like an adjustable rate mortgage with a fixed time-frame to renegotiate. This has been the standard contract offered by Canadian banks since the late 1960's. Most contracts also had 25 year amortization periods.

2.3. **Pricing and negotiation.** The large Canadian banks operate nationally and post prices that are common across the country on a weekly basis in both national and local newspapers, as well as online. There is little dispersion in posted prices, especially at the big banks: the coefficient of variation on posted rates for the Big six is close to zero.

In contrast there is a significant amount of dispersion in transaction rates. In Allen et al. (2011) we document that the coefficient of variation in margins between 1999 and 2004, for example was was 60%. Approximately 10% of borrowers pay the posted rate. The remainder receive a discount below the posted price. This comes about because borrowers can search for and negotiate better rates. One option for borrowers is to visit local branches and negotiate directly with branch managers who have the authority to offer borrowers discounts below the posted price under general guidelines from headquarters. Local branch managers compete against rival banks, but not against other branches of the same bank. Survey evidence from the Canadian Association of Accredited Mortgage Professionals reports that about 55% of new home buyers (new home buyers are defined as individuals who get a new mortgage as opposed to homeowners who refinance or renew an existing mortgage) visit multiple lenders when shopping for a mortgage, but the remainder visit only one.[8]

Alternatively borrowers can hire brokers to search for the best rates on their behalf. Unlike in the United States, brokers in Canada have fiduciary duties. Brokers are compensated by lenders, but "hired" by borrowers to gather the best quotes from multiple lenders. Detailed survey evidence by Taddingstone in 2005 (MortgageBrokerReport@taddingstone.com) found that brokers on average contact 5.9 lenders for their clients, suggesting they do, in fact, assist in gathering multiple quotes.

[5]The reference parameters are set by the Federal Government. See Traclet (2005) for more details.

[6]This is true during our sample. Today borrowers with less than 20% equity must purchase insurance.

[7]Gross debt service is defined as principal and interest payments on the home, property taxes, heating costs, annual site lease in case of leasehold, and 50 per cent of condominium fees. Total debt service is defined as all payments for housing and other debt.

[8]In the U.S., LendingTree reports that 39% of new home buyers gather only one quote. See also Lee and Hogarth (2000).

TABLE 1. Distribution of financial services between main and secondary institutions

Account	Main FI	Second FI	All other FI
Mortgage (all)	67.4%	10.9%	21.7%[a]
Mortgage (no broker)	70.3%	10.8%	18.9%
Mortgage (broker)	37.3%	30.6%	32.1%
Loan	55.8%	9.6%	34.6%
Credit card	77.9%	20.7%	1.4%
GIC or term deposit	72.8%	15.8%	11.4%
Bonds, t-bills, GI's	45.3%	7.8%	46.9%
Mutual funds	38.8%	7.2%	54.0%

Source: Canadian Finance Monitor survey conducted by Ipsos-Reid, between 1999 and 2007.

2.4. **One-stop shopping.** The evolution of the banking system following the 1992 Bank Act revisions led many Canadian households to treat their primary bank as a "one-stop shop", where they purchase the majority of their financial services. From Table 1 we see that 67 per cent of Canadian households have their mortgage at the same financial institution as their main checking account. In addition, 55 per cent of household loans, 78 per cent of credit cards, 73 per cent of term deposits, 45 per cent of bonds/guaranteed investments and 39 per cent of mutual funds are held at the same financial institution as the households main checking account.

3. DATA

3.1. **Mortgage-contract data.** Our data set is a sample of insured mortgage contracts obtained directly from CMHC.[9] We obtained a 10 % random sample of new mortgage contracts issued between 1992 and 2004, sampled by Census Metropolitan Area (CMA).[10] We further restrict the sample to contracts signed within a year of the merger. This is in part for convenience, since our branch data are annual. In addition, we know that the merged entity did not start closing duplicate branches until approximately a year after the official merger date. Our analysis therefore focuses on the short-run impact of the merger, holding fixed the distribution of branches.

We have access to 20 household/mortgage characteristics, including all of the financial characteristics of the contract (i.e. rate, loan size, house price, debt-ratio, risk-type), the lender identity (for the 12 largest lenders), and some demographic characteristics (e.g. income, prior relationship with the bank, residential status, dwelling type).[11] In addition, we observe the location of the purchased house up to the forward sortation area (FSA). This unit of aggregation corresponds to about 4 to 6 census-tracts in urban areas (or between 10,000 and 40,000 households), or one small town in more rural areas. The median population size per FSA is about 16,000.[12]

[9]Although we also have access to Genworth contracts, which we use in Allen et al. (2011) to highlight the importance of discounting in the Canadian mortgage market, the Genworth data does not include contracts from Trust companies, and therefore is of limited use for our merger analysis.

[10]Breslaw et al. (1996) have previously used this data to study mortgage term and amortization choice in the 1980s.

[11]Table 9 in the Appendix lists all of the variables included in data-set.

[12]The FSA is the first half of a postal code. There are over 1,300 FSA's in Canada, and over 850,000 postal codes.

TABLE 2. Summary statistics on mortgage contracts and household characteristics

VARIABLES	Mean	Std-dev.	P_{25}	P_{50}	P_{75}
Transaction rate (p_i)	7.17	0.52	6.83	7.06	7.56
Monthly payment	950.08	401.50	636.83	883.99	1198.11
Income (x 1000$)	66.69	25.99	48.02	62.84	80.17
Loan (x 1000$)	131.00	56.28	87.11	121.79	166.55
Loan-to-value	0.91	0.04	0.90	0.93	0.95
CREDIT $\geq$ 600	.637	.481	0	1	1
Renter	.698	.459			
Parents	.0784	.269			
Broker	.254	.435			
Switcher	.34	.474			

The sample size is equal to 17,074. It includes a random sample of homogenous term and amortization contracts insured by CMHC within one year of the merger. Transaction rate is expressed in difference from weekly average (scaled up by the unconditional average). Renters and parents correspond to new home buyers (the omitted category is home owners). Switcher is an indicator variable equal to one if consumers have no prior experience with the chosen financial institution. The sample is restricted to households with five to eight lenders located within five kilometers of their FSA centroid (see section 2.1 for more details).

During our sample period, some of these characteristics were missing for a fraction of contracts (mostly risk, residential status, and financial intermediary). We include these observations in the regression analysis, but add a series of interaction terms with group dummy variables to control for the fact that the number of missing values was decreasing over time and unequal across lenders (see footnote 20).

We restrict our sample to newly issued mortgages, excluding home-owners that are either refinancing or renewing their mortgage contract. We also focus on contracts with homogenous lengths and terms. In particular, our analysis focuses on contracts with a 25 years amortization period, and 5 year fixed-rate term.[13]

Table 2 describes the key variables that we use in our analysis. The key outcome variable is the transaction interest rate paid by consumers. In most of our regression analysis, we study the impact of the merger on the relative interest rate paid by consumers, by subtracting the average transaction rate among contracts signed during the same week.[14] In Table 2 we scale up this variable by adding the unconditional average rate between 1991 and 2004 (i.e. 7.15%). Interestingly, the week-to-week variation corresponds to 48% of the total variance in interest rates: the standard-deviation goes from 0.74 to 0.52 when we express rates in deviation from their weekly average.

[13]This sample selection choice, however, could bias our estimates if banks reacted to the merger by offering more aggressive discounts on short terms contracts, or variable interest rates. In results not reported here we formally test the null hypothesis of exogenous contract choice with respect the merger, and fail to find evidence of endogenous selection.

[14]The results are unchanged if we use transaction rates in levels and include week fixed effects in our analysis. We chose to use interest rate deviations in order to compare results across different estimation methods which cannot easily accommodate week fixed effects.

Therefore, slightly more than half of the total interest rate variation in our data is coming from cross-sectional dispersion. In comparison the average margin, measured relative to the 5-year government bond rate, is equal to 1.08 during our sample period. This magnitude of dispersion is large compared to other financial markets, considering the homogeneity of the contract terms. For instance, Hortaçsu and Syverson (2004) show that the cross-sectional standard-deviation among mutual-fund transaction fees was equal to 60 bps in 2001. Mutual-funds are much more heterogeneous than the 5-year fixed rates mortgage contracts that we study.

The average home owner in our data earns $67,000, and contracted a loan of $131,000. A large fraction (40%) of households are constrained by the minimum down-payment requirement of 5% of the house price. Also, most consumers represent relatively low default risks to the CMHC, since 65% of borrowers have credit scores greater than 600. This is partly due to the fact that households are constrained to have a total debt service ratio below 40%.

In our sample 25% of contracts were negotiated through a broker, and 34% of consumers switch to a financial institution with whom they did not have any prior experience. Notice that the vast majority of broker transactions are labeled "switchers" (i.e. 74%), since brokers most commonly deal with smaller institutions (i.e. trust or insurance companies). This is because some of the major banks refuse, sometimes explicitly, to deal with brokers.[15] Since we do not observe the number of quotes each consumer receives we use these two variables to proxy for the search behavior of consumers.

3.2. **Lender location data.** We have compiled branch-location information for all financial institutions in Canada from the Financial Services Canada directory produced by Micromedia ProQuest. This data-set provides detailed information at the local-market level and so allows us to identify which local markets were affected by the merger, and to construct concentration measures. For more details on the evolution of branch networks in Canada see Allen, Clark, and Houde (2008).

4. Theory

The impact of a merger on transaction rates can be understood through the lens of a simple Nash bargaining model in which lender j negotiates with borrower i over an interest rate p_{ij}. Assume for simplicity that lenders have a symmetric cost c_i of lending to consumer i, then lender j earns a profit equal to $\pi_{ij} = p_{ij} - c_i$ from the transaction, and zero otherwise.

Borrower i's surplus from dealing with lender j is given by $v_{ij} = \theta_j - p_{ij}$, where θ_j can be thought of as the "quality" of the complementary services offered by j. The bargaining outcome depends on the consumer's outside option, denoted by $v_{i0} = \theta^0 - p_i^0$. This value depends on market structure, on consumers' negotiation ability, and on their utility of owning a house relative to renting.

We assume that consumers are differentiated in their ability to negotiate a discount on their mortgage, which we denote by u_i. For instance, u_i can be thought of as the expected number

[15] Brokers in Canada are compensated by lenders an amount equal to 1-1.3 per cent the value of the mortgage. Unlike in the United States, mortgage brokers are not compensated as a function of the interest rate. Furthermore, mortgage brokers have fiduciary duties with respect to the borrower.

of quotes that a consumer would gather if he/she searched. Rather than explicitly modeling the search process, we assume that both parties expect that a consumer of type u_i will be able to obtain a price quote equal to $p_i^0 = c_i + \mu(u_i)$ if negotiations fail with bank j; where $\mu(u_i)$ is the markup obtained by getting quotes from other banks.[16] Further, we assume that the markup function is weakly decreasing in u_i, and converges to a constant $\bar{\mu}$ for consumers with low bargaining abilities. For these consumers, it is natural to expect that the transaction price is constrained by an upper bound given by the minimum of the common posted interest rate, and their willingness to pay for owning a house.[17]

Assumption 1. *The markup function for a consumer of type u_i satisfies the two properties:*

$$\frac{\partial \mu(u_i)}{\partial u_i} \leq 0, \text{ and } \mu(u_i) = \bar{\mu} \quad \forall \quad u_i < \bar{u}.$$

Assuming an interior solution and equal bargaining weights, the Nash-Bargaining price is equal to:

$$p_{ij}^* = c_i + \frac{1}{2}(\theta_j - \theta^0) + \frac{1}{2}\mu(u_i). \tag{1}$$

This equation is the basis of our empirical analysis. In what follows we formulate a series of predictions as to the impact of losing a lender option on the Nash-Bargaining price.

The first-order effect of losing a lender option is to reduce the ability of consumers to obtain larger discounts by gathering multiple quotes. If the merger only induces a market power increase, it can be thought of as changing the shape of the markup function from $\mu(\cdot)$ to $\mu^M(\cdot)$. The average treatment effect can thus be written as:

$$\bar{\alpha} = E\left[p_{ij}|M=1\right] - E\left[p_{ij}|M=0\right] = \frac{1}{2}E\left[\mu^M(u_i) - \mu(u_i)\right] \geq 0. \tag{2}$$

This leads to our first prediction: If the number of lenders in a consumer's choice-set affects the discount he/she is able to extract, the average treatment effect should be positive.

We are also interested in testing whether the merger effect varies across borrowers. Specifically, if $\mu(u_i)$ is a constant for consumers with poor negotiation abilities or high search costs, the merger should lead to differential price increases for consumers with high and low u_i's.[18] In particular, the treatment effect of the merger should be equal to zero for all consumers with $u_i < \bar{u}$:

$$\alpha_i = \mu^M(u_i) - \mu(u_i) = \bar{\mu} - \bar{\mu} = 0. \tag{3}$$

This is our second prediction: Assuming that borrowers have heterogeneous search and bargaining abilities, those with poor negotiation abilities or high search costs will not be affected by the merger.

[16] See Bester (1993) and Desai and Purohit (2004) for models of price negotiation in which $\mu(u_i)$ is an endogenous outcome.

[17] In principle $\bar{\mu}$ should be consumer specific. We use this notation to highlight the fact that these consumers obtain a discount that is independent of market structure, and constant across lenders.

[18] Note that a similar effect could arise for consumers with very high u_i's since both before and after the merger they should be able to drive the markup to (or close to) zero.

Finally, we also consider two indirect effects of the merger. First, the merger can increase the willingness to pay of consumers for the new entity: $\theta_{AB} > \max\{\theta_A, \theta_B\}$, where A is the Bank and B is the trust and AB is the merged entity. If this is the case, the average treatment effect of the merger will be different for AB consumers (direct effect) and competing banks' consumers (indirect effect). For instance if $\theta_j = \theta$ for all j and $\theta_{AB} > \theta$:[19]

$$\begin{aligned}\alpha_{AB} &= E\left[p_{ij}|M=1, j_i = A, B\right] - E\left[p_{ij}|M=0, j_i = A, B\right] = \frac{1}{2}(\theta_{AB} - \theta) + \frac{1}{2}E\left(\mu^M(u_i) - \mu(u_i)\right) \\ &> \frac{1}{2}E\left(\mu^M(u_i) - \mu(u_i)\right) = E\left[p_{ij}|M=1, j_i \neq A, B\right] - E\left[p_{ij}|M=0, j_i \neq A, B\right] = \alpha_{\text{Other}}.\end{aligned}$$

This yields our third prediction: If the merger increases the willingness to pay of consumers for the new entity, then the direct effect of the merger is larger than the indirect effect.
Note from the perspective of borrowers transacting with a competing lender, the effect of the merger is to improve the quality of their outside option and therefore put downward pressure on the rate quote from their lenders.

A similar asymmetry between lenders can be caused by a sorting effect: the distribution of consumer types within A or B pre-merger is different than within AB post-merger. For instance, it is possible that consumers dealing with trust companies are more price sensitive (i.e. higher u_i) than consumers dealing with national banks. Unless we observe u_i, we cannot distinguish between a quality improvement and a change in the mix of consumer types across lenders. In the empirical analysis, we use proxy variables for u_i to empirically distinguish between these two sources for heterogeneous treatment effects. For instance, we estimate the effect of the merger on the probability of using a broker, or of remaining loyal to the home bank.

A final consequence of the merger that would affect lenders symmetrically is an increase in the search effort of consumers. This can be thought of as an increase in the number of quotes that a consumer of type u_i is expected to gather, or equivalently, as a rightward shift in the distribution of u_i:

$$\Pr(u_i < x|M=1) \leq \Pr(u_i < x|M=0). \tag{4}$$

This is our final prediction: The merger increases the search effort of consumers.
This sorting effect of the merger attenuates the market power increase, and therefore reduces the magnitude of the average treatment effect $\bar{\alpha}$.

5. Estimation and identification strategy

5.1. **Estimation strategy.** Our analysis focuses on one of the major acquisitions that took place in Canada between the mid 1990s and early 2000s. For confidentiality reasons we label the two institutions A and B. Firm A is a national bank, and therefore present in nearly all local markets in Canada prior to the merger. Firm B, on the other hand, is smaller, and prior to the merger

[19] A similar asymmetry can be interpreted as efficiency gains resulting from the merger. In the current setting, quality improvements and cost reductions are isomorphic.

less than 2 per cent of firm B's observed contracts were in markets where A was not one of B's competitors.

Our empirical strategy relies on the idea that when two banks merge, the pre-merger location of branches creates discrete changes in the structure of local markets, and therefore the choice-set of consumers. In particular, when two neighboring branches merge because of a national acquisition/merger, local competition is immediately reduced since local branch administrators part of the same network stop competing. Importantly, since bank mergers are negotiated nationally, these changes can be viewed as exogenous relative to local market conditions, at least in the short run.

In order to define a set of consumers affected by the merger, we assume that consumers shop for mortgages in a neighborhood around the house they purchase. This defines their choice set. In our context, although we observe each household only once, we observe most neighborhoods, defined by postal codes, before and after the merger. However, rather than study the effect of the merger at the postal-code level, we study its effect at the contract level. We therefore index everything by i where i captures three things: (i) the individual borrower (or household), (ii) the time period of the contract (before or after the merger), and (iii) the location of the purchased house.

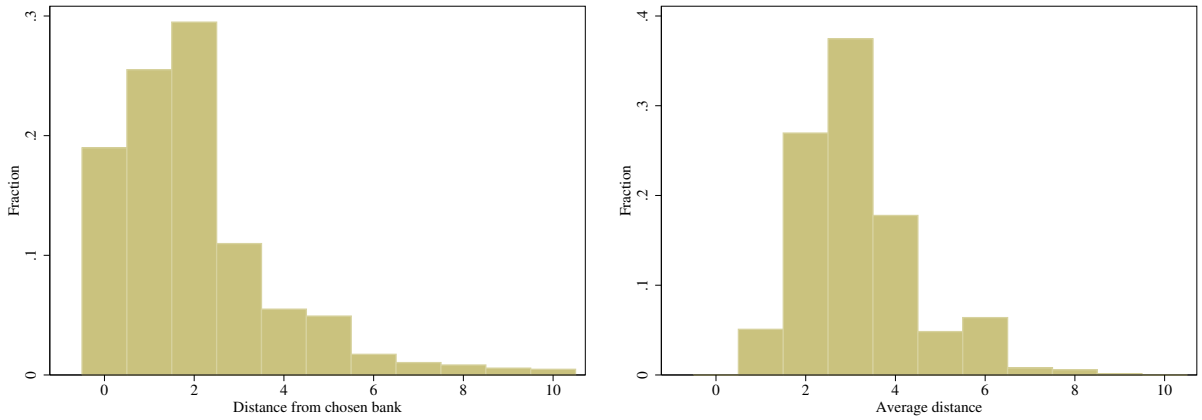
Treated consumers are therefore defined as purchasing a house in a neighborhood in which both A and B were present prior to the merger, and consumers located in neighborhoods with only A or B, or none are part of the control group. We let G_i denote the treatment group indicator. Similarly, T_i is a period indicator variable equal to one for contracts signed after the merger, and $M_i = T_i \cdot G_i$ is the treatment (merger) indicator variable.

Since we observe a panel of contracts before and after the mergers, we can estimate the effect of the merger on transaction rates using a standard difference-in-difference estimator:

$$p_i = \alpha M_i + \beta T_i + \gamma G_i + \lambda' \mathbf{Z}_i + u_i, \tag{5}$$

where α measures the average effect of the merger on transaction interest rates (i.e. $\bar{\alpha}$ in equation 1), and $\mathbf{Z}_i$ is a vector of control variables describing the financial and demographic characteristics of the contract (i.e. including month and province indicator variables). It includes variables that we believe are predetermined at the negotiation stage (i.e. financial, market, and demographic characteristics), and excludes variables that are jointly determined through a negotiation process (i.e. broker transactions, and relationship duration indicator). We also include in $\mathbf{Z}_i$ time-varying variables measuring the distribution and number of branches among competing networks to account for changes in the number retail branches over the period.[20]

[20]The exact set of control variables are: income, loan size, loan to income ratio, other debts, debts to income ratio, loan-to-value categorical variables, credit score categories (4), residential status category (4), FSA demographic characteristics (i.e. income, house value, fraction of renters, fraction with university degree, fraction of inter-provincial migration, age), number of lenders (other than A or B), and average number of branch per lender (other than A or B). To control for the non-random nature of missing household characteristics we interact a missing value dummy with: treatment group, after merger, and bank indicator variables.



(A) Distance to chosen bank (km)

(B) Distance to competing banks (km)

FIGURE 2. Distribution shortest distances between homes and banks

To estimate heterogenous treatment effects, such as the direct and indirect effect described above, we allow α to be a linear function of X_i:

$$p_i = \alpha M_i + \alpha_X M_i \cdot X_i + \beta T_i + \gamma G_i + \beta_X T_i \cdot X_i + \gamma_X G_i \cdot X_i + \lambda' \mathbf{Z}_i + u_i, \tag{6}$$

and $\alpha + \alpha_X X_i$ measures the average treatment effect conditional on X_i. Both parameters can be estimated by OLS. We also estimate equation 5 using other outcome variables to measure the sorting effects of the merger (i.e. choice of bank, broker, and switching probability).

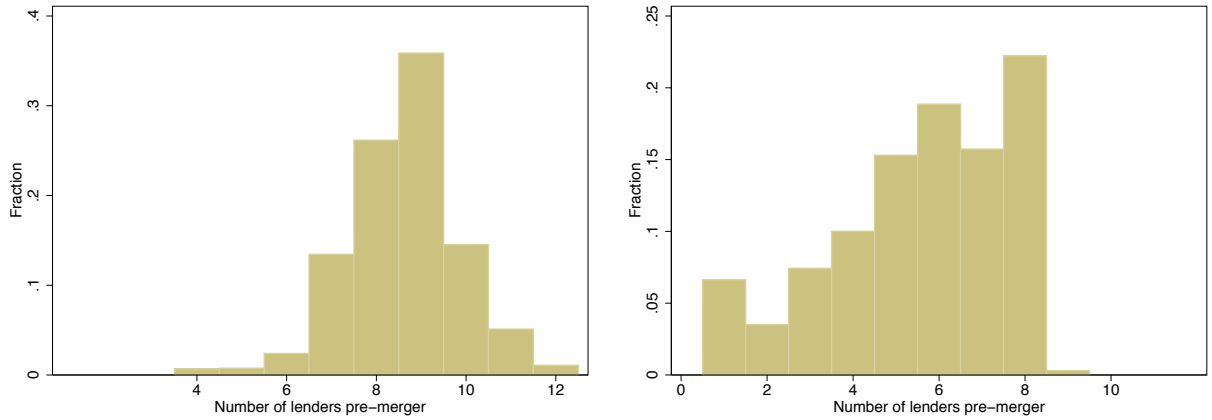
Below we discuss three issues related to estimation of the treatment effect of the merger: (i) the definition of a neighborhood size, (ii) the identification assumptions, and (iii) the estimation of the distribution of the treatment effects.

5.2. **Choice-set boundaries.** We assume that consumers shop for mortgages in a neighborhood of radius 5 KM around the center of the house they purchased.[21] This assumption appears to be reasonable in the case of mortgage contracts since consumers transact with an institution that is on average located within 2 kilometers of the center of their forward sortation area.[22] The two histograms in Figure 2 show that the average distance to consumers' chosen lenders is much smaller than the average distance to other financial institutions.

5.3. **Identification assumptions.** Unlike other forms of program evaluation, retail mergers are not endogenously chosen by local market participants. This does not mean that the treatment is necessarily independent of unobserved transaction attributes u_i, since the timing and location of local mergers can be correlated with other aggregate variables affecting interest rates. The key identifying assumption that allows us to interpret α (and α_X) as the causal effect of the merger, is that conditional on $\mathbf{Z}_i$, differences between the treatment and control groups are the same on

[21] In section 6.4 we relax this assumption and evaluate the robustness of our results to the size of competing neighborhoods.

[22] We define distance as the Euclidian distance between the center of each forward sortation area of the household and the closest branch, by postal code, associated with the chosen institution.



(A) Markets with A & B

(B) Markets with A or B alone (or none)

FIGURE 3. Distribution of the number lenders per local markets before the merger

average in different time periods (pre- and post-merger). That is, the merger is not *confounded* with aggregate trends specific to the control or treatment groups. In the simple difference-in-difference estimator, the control group observations are therefore used to linearly predict the evolution of those aggregate variables, and construct the missing counter-factual outcome: $E(p_i|T_i = 1, G_i = 1, M_i = 0, \mathbf{Z}_i)$.

An often overlooked issue in the evaluation of mergers is the comparability of treatment and control groups. For instance, Heckman et al. (1997) note that "failure to locate participants and comparison group members in the same labor market is a major source of evaluation bias", even more so than selection bias caused by endogenous participation. Although controlling for observable differences by including $\mathbf{Z}_i$ in the regressions solves part of this problem, the linear specification imposes very strong restrictions when the two groups are not comparable. When treated and control observations are too different, the linear specification will fail to accurately predict the evolution of rates in the treated markets absent of the merger.

In our context, the most important difference is that local markets with both A and B tend to be larger and have more lenders. Figures 3a and 3b illustrate that the two groups overlap only in medium-size markets with four to nine lenders. Less than 1% of control markets have more than 8 lenders, while less than 1% of treatment markets have fewer than 5.

Our main approach to deal with this issue is to restrict the sample to local markets with five to eight lenders, and to estimate equation 5 by OLS. This restriction comes at the cost of weakening the external validity of our estimates, since we can only measure the effect of a merger in markets with a moderate level of competition. With this additional restriction, households with only one or none of the two firms are under-represented (i.e. 37% versus 67%), but less so than in the full sample (i.e. 10% versus 90%). The final sample includes 17,074 observations over 427 different locations (including 265 with both institutions).

Table 3 describes the structure of choice-sets, defined as the 5 KM buffer around each observed contract. The first two columns illustrate the distribution of the number of lenders and branches

TABLE 3. Summary statistics on consumer choice-sets prior to the merger

	Full sample		$5 \leq N \leq 8$	
	A or B or none	A & B	A or B or none	A & B
Nb. Lenders	5.575	8.655	6.681	7.527
	(2.14)	(1.25)	(1.14)	(0.68)
Nb. Branches	17.742	62.988	23.087	28.031
	(20.60)	(70.68)	(21.94)	(18.45)
Branch HHI	1.538	1.674	1.621	1.622
	(0.31)	(0.32)	(0.28)	(0.25)
Share A	0.060	0.142	0.057	0.161
	(0.09)	(0.06)	(0.07)	(0.06)
Share B	0.008	0.098	0.010	0.117
	(0.06)	(0.06)	(0.05)	(0.05)
Δ Branch HHI		0.187		0.213
		(0.22)		(0.25)

Each entry corresponds the sample average and standard-deviation (in parenthesis), calculated using the observation weights from the mortgage contract data-set. Local markets are defined as 5 KM euclidian distance around each FSA centroid. Markets "A or B, or none" do not have A and B together, and markets "A & B" have both merging parties.

in the full sample; including markets with more than 8 and less than 5 lenders. Excluding the non-overlapping local markets leads to comparable neighborhoods: the average number of branches and Herfindahl-Hirschman Index (HHI) are similar across the two groups, as is the number of lenders pre-merger. The last three rows show that, absent of any other simultaneous change, concentration would increase significantly in markets with both A and B. Both institutions had a large presence in A & B markets, with a cumulative average market share of 27%. The merger alone therefore corresponds to an average increase of 0.213 points in the branch HHI.

Our second approach to deal with the comparability of treated and control neighborhoods is to use the difference-in-difference matching estimator proposed by Heckman et al. (1997, 1998). This estimator computes the counter-factual average interest rate changes in the treatment group, using a weighted average of changes observed in the control group. The weights used for averaging are estimated with a kernel, and observations that are "similar" to the corresponding treated observations receive larger weights.

In our context, although we observe each household only once, we observe most neighborhoods before and after the merger. We therefore use a kernel-based propensity score estimator, and compare contracts according to the probability that their neighborhood was part of the treatment group. To calculate the propensity score, we estimate a Logit model that controls for time-invariant demographic characteristics and branch concentration of each neighborhood. Moreover, we compare neighborhoods with the same number of lenders pre-merger. By construction this restricts our analysis to markets with five to eight lenders.

This leads to the following estimator of the average treatment effect on treated consumers:

$$\hat{\alpha}_M = \frac{1}{|\mathcal{N}_{1,1}|} \sum_{i \in \mathcal{N}_{1,1}} \left\{ p_i - \sum_{j \in \mathcal{N}_{0,1}} \omega_1(i,j) p_j \right\} - \frac{1}{|\mathcal{N}_{1,0}|} \sum_{i \in \mathcal{N}_{1,0}} \left\{ p_i - \sum_{j \in \mathcal{N}_{0,0}} \omega_0(i,j) p_j \right\}, \tag{7}$$

where $\mathcal{N}_{G,T}$ denotes the set of observations in group $G = \{0,1\}$ (i.e. $G = 1$ for treatment group) and period $T = \{0,1\}$ (i.e. $T = 1$ after the merger), $|\mathcal{N}_{g,t}|$ measures the number of observations in (G,T), and $\omega_T(i,j) = K_b(i,j)/\sum_{j \in \mathcal{N}_{0,T}} K_b(i,j)$ is a weight measured with a kernel with bandwidth b over the propensity score of observations i and j. Note that $K_b(i,j) = 0$ if i and j do not face the same number of lenders per-merger. To ensure a common support in the propensity score distribution across the two groups, we drop observations for which the propensity score kernel density is below the 5% percentile of the positive density.[23]

5.4. **Distribution of treatment effect.** An important criticism of the standard difference-in-difference model is that it imposes a strong functional form assumption: equation 5 assumes that transaction interest rates are a linear and additive function of persistent group-level heterogeneity (i.e. β) and time-varying aggregate shocks (i.e. γ). This criticism has been articulated by Athey and Imbens (2006), among others, who propose an alternative non-parametric estimator for discrete treatments that allows for heterogenous and non-linear effects.

To recast their model in our framework, we use the pricing equation from the Nash-Bargaining model derived in section 4, and assume that the common lending cost and bank quality is $c_i + \frac{1}{2}(\theta_j - \theta^0) = \lambda' \mathbf{Z_i}$. This leads to a semi-parametric pricing equation:

$$p_i = \lambda' \mathbf{Z_i} + h(u_i, T), \tag{8}$$

where, in the context of the Nash-bargaining model described above, $h(u_i,T) = \frac{1}{2}\mu(u_i,T)$ and $T \in \{0,1\}$ incorporates an aggregate time trend measuring for instance the diffusion of brokers into the market, or aggregate business cycles.[24]

Recall that the merger affects the outside option of consumers by making it harder to negotiate large discounts. The merger effect for consumer u_i is thus equal to:

$$\alpha(u_i) = h^M(u_i,T) - d(u_i,T), \tag{9}$$

where $h^M(u,T)$ measures the markup that consumer u_i pays in period t with the merger. Moreover, if the market power increase impacts consumers with varying negotiation skills differentially, we should expect a distribution of treatment effects: $\alpha(u_i) \neq \alpha(u_i')$ if $u_i \neq u_i'$.

In order to estimate the distribution of merger effects, we impose three assumptions discussed in Athey and Imbens (2006).

Assumption 2. *The markup function $d(u,T)$ is strictly monotonic in u_i, and constant across groups.*

[23]To control for time-varying observed characteristics Z_i, we implement the regression-adjusted estimator proposed by Heckman, Ichimura, and Todd (1997). We first project rates on the contract characteristics, month fixed effects, and provincial fixed effects using the control group observations. The results are mostly unchanged if we use an unadjusted specification. Standard-errors are calculated by bootstrapping.

[24]The vector $\mathbf{Z}_i$ includes bank fixed effects, which absorb permanent differences in the quality of each lender.

Assumption 3. *The distribution of types $u_i \sim H_G(u)$ differs across groups G, but is constant over time.*

Assumption 4. *The support of u_i in the treatment group overlaps with the support of u_i in the control group: $\mathcal{U}_1 \subseteq \mathcal{U}_0$.*

Under these assumptions, we can calculate the change in the function $d(\cdot)$ that would have occurred over time without the merger. This transformation is implemented using the fact that the change in the qth percentile of the control price distribution identifies the change in the markup function that is strictly due to time. Using this logic, and the fact that the empirical price distributions are invertible (under assumption 2), we can recover the counter-factual distribution of prices, denoted $\tilde{F}_{1,1}$, in the treatment group for any price p in the common support:

$$\tilde{F}_{1,1}(p) = F_{1,0}\left(F_{0,0}^{-1}\left(F_{0,1}(p)\right)\right), \tag{10}$$

where $F_{G,T}(p)$ is the CDF of prices in the sub-population (G,T). Intuitively, we obtain the counter-factual distribution of prices by transforming the observed price distribution in period zero (i.e. $F_{1,0}(p)$) in a such a way that mimics the change in the price distribution observed in the control group. From this transformation we can recover the average treatment effect on the treated:

$$\begin{aligned} \alpha_C &= E\left[P_i | i \in \mathcal{N}_{1,1}\right] - E\left[\tilde{F}_{1,1}^{-1}(P_i) | i \in \mathcal{N}_{1,0}\right] \\ &\approx \frac{1}{|\mathcal{N}_{1,1}|}\sum_{i\in\mathcal{N}_{1,1}} p_i - \frac{1}{|\mathcal{N}_{1,0}|}\sum_{i\in\mathcal{N}_{1,0}} \hat{F}_{0,1}^{-1}\left(\hat{F}_{0,0}(p_i)\right) = \hat{\alpha}_C, \end{aligned} \tag{11}$$

where $\hat{F}_{G,T}$ corresponds to the empirical CDF of prices in sample (G,T). We use a subscript C to label this the "change-in-change" estimator.

Similarly, we can recover an estimate of the effect of the merger for each percentile of the distribution:

$$\hat{\alpha}_C(q) = \hat{F}_{1,1}^{-1}(q) - \hat{F}_{0,1}^{-1}\left(\hat{F}_{0,0}\left(\hat{F}_{1,0}^{-1}(q)\right)\right). \tag{12}$$

Since we can normalize the scale of u_i to be between zero and one, the previous expression measures the estimated effect of the merger on a consumer with negotiation ability $u_i = q$.

In practice, we construct both estimators while controlling for covariates in the parametric form suggested by equation 8, assuming that u_i is independent of Z_i. We follow the suggestion of Athey and Imbens (2006) (pages 465-466), and construct the empirical price distributions using the residuals of a regression of transaction interest rates on Z_i. The standard-errors are obtained by bootstrapping.[25]

6. Results

6.1. **Average treatment effects.** In Table 4 we present OLS regression results of the average treatment effect of the merger. The dependent variable is measured as the transaction rate minus the weekly average rate across all markets. For most of our analysis we exclude local markets with

[25] See Huynh et al. (2011) for analysis of the bootstrap performance in this context.

TABLE 4. Effect of the merger on transaction prices

VARIABLES	(1)	(2)	(3)	(4)	(5)
Merger	0.00991	0.0593[a]	0.154[a]	-0.0482	0.0908[a]
	(0.0159)	(0.0202)	(0.0482)	(0.0616)	(0.0228)
Merger X Indirect			-0.117[b]		
			(0.0514)		
Merger X A+B branch share				0.696[b]	
				(0.336)	
Merger X 1(LTV=95)					-0.0776[b]
					(0.0340)
Constant	7.456[a]	7.451[a]	7.335[a]	7.434[a]	7.397[a]
	(0.0753)	(0.0848)	(0.0583)	(0.0886)	(0.0846)
Observations	35,352	17,074	17,074	17,074	17,074
R-squared	0.059	0.059	0.060	0.060	0.076

Heteroscedasticity-robust standard-errors are in parenthesis. Significance levels: [a] p-value<0.01, [b] p-value < 0.05, [c] p-value < 0.1. The dependent variable is measured as the transaction rate minus the weekly average across all markets. The sample includes all contracts with non-missing characteristics one year before and one year after the merger. Neighborhoods with fewer lenders than the 1st percentile in the treatment group and more than 99th percentile in the control group are excluded (i.e. less than 5 or more than 8). The control variables in columns (2)-(5) include: income, loan-size, loan/income interaction, loan-to-value categories (3), credit score categories (5), residential status (i.e. parents or renter), census characteristics (income, age, population, house-value), and bank, month and province dummies. Specification (1) uses the same controls as specification (2), but is estimated on the full sample, including local markets with more than eight or less than five lenders.

fewer than 5 and more than 8 lenders. As discussed in section 5.3, we do this in an effort to make our control and treatment markets as similar as possible.

Restricting the sample to overlapping market structures turns out to be important, as illustrated by the results in columns (1) and (2) of Table 4. In these specifications we present the aggregate effect of the merger on rates. Column (1) is estimated on the full sample, including local markets with less than 5 or more than eight lenders, while column (2) is estimated on the restricted sample. Using the full sample we would conclude that the merger had no effect on rates. Once we restrict the sample, we estimate that the merger led to an average interest rate increase in treated markets of around 6 basis points (bps).

Note that an increase of 6 bps corresponds to 12% of the cross-sectional standard-deviation of interest rates, or 5.5% increase in retail margins. Assuming a common loan size and holding it fixed at $100,000, we estimate that the merger led to a $4 increase in monthly payments which translates into $240 over the five years of the contract. Overall this effect is small, and suggests that the market is fairly competitive: the average consumer is able to extract a large share of the transaction surplus through search and negotiation. It should be pointed out, however, that randomness in choice sets of borrowers may introduce some amount of measurement error in

the treatment variable that would bias downward the point estimate. In the robustness section we consider different definitions for the choice set of consumers (different distance measures), but this may not entirely solve the problem since, if a measurement error exists, it likely stems from the fact that there is heterogeneity in the choice set for individual borrowers. For instance 5 kms may be appropriate for urban borrowers, but rural borrowers may consider lenders that are located much further away.

Columns (3) to (5) show that the effect of the merger is heterogeneous across consumers and markets. Column (3) decomposes the aggregate merger effect into a direct effect and an indirect effect. Consumers transacting with the merged entity experience the largest price increase: over 15 bps, compared to less than 4 bps for consumers selecting a competing institution. Again supposing a loan size of $100,000, this works out to an increase of $750 at the merged entity and just $180 at competing institutions. In column (4) we study how the effect depends on the market share of the merged entity's. We find that the average treatment effect is an increasing function of the combined number of branches of the two institutions. Specifically, rates increased by almost 30 basis points in markets where the merged entity controlled 45% of the branches (95th percentile). Over five years this works out to almost $1300 in increased payments. These results are consistent with the idea that the merger raised the value of transacting with the merged entity, either because it raises the value of the complementary services offered by the bank, or reduces the shopping cost for consumers.

The last specification in column (5) suggests that less financially constrained buyers experience larger rate increases as a result of the merger. We attribute this to differences in the bargaining abilities of financially constrained and unconstrained consumers. In general, consumers able to negotiate larger discounts are the most likely to be affected by the merger, since it reduces their ability to gather multiple competing quotes. This result suggests, therefore, that consumers with more flexibility to increase the down-payment benefit the most from competition between lenders. In contrast, financially constrained buyers are less likely to shop around for multiple quotes, or have fewer bargaining options. One possible reason for this is that these consumers perceive that they cannot do any better by searching and in fact may even be rejected as clients (despite the fact that mortgage insurance guidelines are constant across lenders).

In Table 5 we study the robustness of our main results to alternative estimation methods. The first column compares the aggregate effect of the merger estimated using the matching and change-in-change estimators. Overall all three models yield similar conclusions. The matching estimator produces larger effects than the other two (i.e. 7.6 bps compare to 5.9 or 6.4), but all three yield overlapping confidence intervals. The results suggest that matching the sample based on a common number of lenders range, as in Table 4, corrects for most, if not all, of the bias generated by systematic differences across control and treatment groups.

In order to compare the heterogeneous treatment effects across methods, we estimate the model under different sub-samples instead of estimating interaction terms as in Table 5. The matching estimator is less precise in general, and some of the estimates are not significantly different from zero

TABLE 5. Average treatment effect of the merger on transaction prices for different estimators and sub-populations

ESTIMATORS	(1) Aggregate	(2) Direct	(3) Indirect	(4) LTV=95%	(5) LTV<95%
OLS DID	0.059^a	0.128^b	0.041^c	0.017	0.088^a
	(0.020)	(0.054)	(0.022)	(0.034)	(0.024)
Matching DID	0.076^a	0.115	0.053^c	0.016	0.092^b
	(0.029)	(0.073)	(0.031)	(0.046)	(0.038)
Change-in-change	0.063^a	0.133^b	0.051^a	0.018	0.104^a
	(0.018)	(0.053)	(0.019)	(0.028)	(0.024)
Nb. Observations	17,074	3,117	13,957	7,171	9,903
% Treated	0.67	0.76	0.64	0.66	0.67

Point estimates correspond to the average treatment effect on treated calculated using OLS (row 1), propensity score matching (row 2), and the Change-in-Change estimator (row 3). OLS Standard errors are are clustered at the local neighborhood level (i.e. FSA), and matching and CiC standard-errors were obtained by bootstrapping with 1,000 replications. Significance levels: ^{a} p-value<0.01, ^{b} p-value < 0.05, ^{c} p-value < 0.1. The dependent variable is measured as the transaction rate minus the weekly average across all markets. The sample includes all contracts with non-missing characteristics one year before and after the merger. Neighborhoods with fewer lenders than the 1st percentile in the treatment group and more than the 99th percentile in the control group are excluded (i.e. less than 5 or more than 8). The control variables include: income, loan-size, loan/income interaction, loan-to-value categories (3), credit score categories (5), residential status (i.e. parents or renter), and bank, month and province dummies.

at standard levels as the sample shrinks.[26] However, the point estimates are similar to those derived from OLS and the change-in-change estimator, and we do not document any systematic bias across sub-samples. Our conclusions therefore remain unchanged: $A + B$ consumers were more affected by the merger than competing institutions' consumers, and financially un-constrained consumers experienced significant price increases (between 8.8 and 10.4 bps).

6.2. **Sorting effects of the merger.** Part of the heterogeneity in price effects of the merger is associated with the endogenous sorting of consumers. We are interested in distinguishing between two effects. First, the merger can increase the search effort of consumers, which would attenuate the market power effect of the merger. Second, the merger can affect the mix of consumer types that each lender is serving post-merger.

To study these effects, we consider the impact of the merger on the probability of choosing one of the merging lenders, to use a broker, or to switch institution.[27] The results are presented in Table 6. In column (1) we restrict attention to the probability of choosing Bank A. We find that A's share increases by 2.5%, as would be expected since it has absorbed B following the merger. However, their combined share decreased by 1.2% (see column (2)). This decrease is small however, and is

[26]Our matching estimator requires a large sample since we compare contracts within the same market structure category, which sometimes reduces the number of comparable observations to a very small number.

[27]We have also tested for the presence of merger effects on other financial characteristics, but failed to find any statistically significant effects. These additional results are available upon request.

TABLE 6. Sorting and Search-intensity effects of the merger

VARIABLES	(1) 1(Bank=A)	(2) 1(Bank=A,B)	(3) Broker	(4) Switcher	(5) Income	(6) Loan
Aggregate effect:						
Merger	-0.0220	-0.0676[a]	0.0567[a]	0.0636[a]	0.0236[b]	0.0264
	(0.0162)	(0.0174)	(0.0196)	(0.0228)	(0.00942)	(0.0196)
Disaggregate effect:						
Merger X Direct			-0.151[a]	0.0435[c]	0.0207	-0.00519
			(0.0343)	(0.0231)	(0.0236)	(0.0482)
Merger X Indirect			0.0925[a]	0.0476[c]	0.0228[b]	0.0377[c]
			(0.0236)	(0.0254)	(0.0109)	(0.0220)
Observations	11681	11681	11681	11660	11681	11681

Heteroscedasticity-robust standard-errors are in parenthesis. Significance levels: [a] p-value<0.01, [b] p-value < 0.05, [c] p-value < 0.1. The sample includes all contracts with non-missing characteristics one year before and after the merger. Neighborhoods with fewer lenders than the 1% percentile in the treatment group and more than 99% percentile in the control group are excluded (i.e. less than 5 or more than 8). The control variables include: bank, month and province dummies.

not statistically different from zero. This is despite the fact that transaction rates increased more among $A+B$ consumers, than among competing lenders. This is consistent with our interpretation that the quality of the merged entity improved (its branch network and service range expanded).

A complementary interpretation is that the merged entity began serving a different mix of consumers after the merger. Indeed, part of the direct price increase that we document is due to the fact that prior to the merger Trust B consumers were more likely to obtain large discounts (conditional on their Zs, i.e., observable characteristics).[28] These price sensitive types are more likely to transact with competing institutions post-merger, which would lead to an asymmetric price change. Since the aggregate market share of the new entity did not change significantly, these results suggest that Bank $A + B$ managed to attract new types of consumers to compensate; most likely a larger fraction of consumers with a high valuation for the range of services or the network size of their mortgage lender.

The results in columns (3) and (4) confirm in part this interpretation. First, both the merged entity and competing banks are significantly more likely to receive new consumers (i.e. consumers with no prior experience with their mortgage lender). Second, the merged entity is significantly less likely to transact with brokers, which is consistent with the idea that price sensitive consumers have been switching away from the merged entity. In comparison, competing institutions increased their share of broker transactions by 9%. In other words, following the merger borrowers that engage in search by employing a broker are less likely to deal with the merged entity. This

[28]Excluding B consumers from the sample reduces the direct effect in column (3) of Table 4 to 9 bps.

could either be because the merged entity is not offering competitive rates to brokers, or because its more dominant position in the market allows it to refuse to deal with brokers altogether.[29]

Moreover, in the aggregate sample (i.e. first row), the probability of switching institutions or using a broker increased by nearly 6%. This offers indirect support for our prediction that consumers are more likely to search aggressively for quotes after the merger; either through a broker or individually. While we do not observe the number of quotes that consumers receive, it is very likely that most consumers start their search process by obtaining a quote from their home institution. Indeed according to a 2009 survey by the CAAMP, 80% of new home buyers shopped at their home institutions. Therefore, the increase in the switching probability suggests that consumers are more likely to gather multiple quotes because of the merger. More intense search on the part of borrowers may explain in part why the average treatment effect is relatively small.

6.3. **Distribution of treatment effect.** Finally, we analyze the distribution of treatment effects across consumers' unobserved types. As discussed in Section 5.4, since we condition on a rich set of characteristics, the percentiles of the distribution can be interpreted as the unobserved negotiation ability of consumers. Low quantiles are borrowers who received a relatively big discount (e.g. searchers), while high quantiles are borrowers paying high rates (e.g. non-searchers). In this context, our Nash bargaining model predicts that non-searchers should not be affected by the merger since regardless of market structure they pay the upper bound of the markup function.

Figure 4 presents the distribution of the average treatment effect of the merger on transaction prices using the change-in-change estimator. The aggregate effect becomes statistically indistinguishable from zero around the 70th percentile. Moreover, this gives a measure of the fraction of consumers unable to haggle (i.e. $\bar{u}$ in the theory section). Recall, that an independent survey of consumers' shopping habits revealed that 45% of consumers gather only one price quote when shopping for their first mortgage. Assuming that these consumers are at the top of the price distribution, our results suggest that around two thirds of consumers who accept the initial quote do so because they are unwilling or unable to gather competing offers. This does not mean however that these consumers pay the posted interest rate, since we estimate that only 15% of consumers pay a rate within 10 bps of the posted price. Note also that we do not report results for this very top part of the distribution. There are two reasons for this. First, there are many outliers in our data in this part of the distribution. Second, the change-in-change estimator does not work here since the empirical price distribution is not invertible at the posted price.

In Table 7 we present estimates of the treatment effects at different percentiles of the distribution for several specifications in addition to the aggregate effect. Results are calculated using the change-in-change estimator, and we report the treatment effect at six percentiles between 20 and 80. As in Figure 4 we find that the effect of the merger ranges from around 6 to 8 bps for borrowers in the 20th to 60th percentiles percentiles of the conditional transaction rate distribution, but is statistically indistinguishable from zero in the 80th percentile. In rows (2) through (5) we decompose the aggregate result, and study again the effect of the merger on different sub-populations. The results show that the same non-linear pattern that we observed in the aggregate sample emerges

[29] At least one national bank has at times explicitly refused to deal with brokers.

TABLE 7. Distribution of treatment effects of the merger on transaction prices

GROUPS	Nb. Obs.	% Treated	Distribution of treatment effects 20%	40%	50%	60%	80%
Aggregate	11,428	0.67	0.0873^b	0.0816^b	0.0838^b	0.0840^b	0.0482
Direct	2,636	0.76	0.1765^b	0.1874^b	0.1808^b	0.2056^b	0.1119
Indirect	8,792	0.64	0.0798^b	0.0685^b	0.0627^b	0.0677^b	-0.0294
LTV$=95\%$	4,686	0.66	0.0350	0.0426	0.0545	0.0834	0.0468
LTV$<0.95\%$	6,742	0.67	0.1193^b	0.1122^b	0.1035^b	0.0788^b	0.0123
N<8	5,826	0.80	0.0650^b	0.0864^b	0.0897^b	0.0986^b	0.0662

The treatment effects at different percentiles of the distribution are calculated using the Change-in-Change estimator (see text). The dependent variable is measured as the transaction rate minus the weekly average across all markets. The sample includes all contracts with non-missing characteristics one year before and after the merger. Neighborhoods with fewer lenders than the 1% percentile in the treatment group and more than 99% percentile in the control group are excluded (i.e. less than 5 or more than 8). Confidence intervals were calculated by bootstrapping the sample 1,000 times. A b superscript is added to indicate estimates that are significantly different from zero at the 5% level.

for the direct and indirect effects of the merger which are measured to be around 19 and 7 bps respectively in the lower and middle percentiles, and lower in the top 30%. The decrease is more important in the competing banks subsample, and the direct effect is significantly different from zero for all percentiles. Similarly, we measure the effect of the merger to be about 10 bps for less financially constrained borrowers in the lower and middle percentiles, and zero in the top part of the distribution.

It is important to note that these borrowers at the top of the distribution are not subject to higher rates because they would not qualify for a loan at another bank given unobserved characteristics. As mentioned in Section 2.2, borrowers qualifying at one bank, will also qualify at other institutions given that the lender is protected in the case of default. It should also be noted that for these results to be meaningful, the mix of consumer types must be constant across sub-groups. Specifically, the distribution of search costs across sub-populations must be the same over time, which is only the case if the merger does not lead borrowers with different search costs to sort themselves endogenously into different sub-populations. Otherwise, Assumption 3 would be violated. This restriction is mostly problematic when we look at the direct and indirect effect of the merger.

Our results are consistent with those of Sapienza (2002) who, in the context of business lending, finds that those borrowers with either many or very few outside options for loans (as measured by the number of other banking relationships they have) are unaffected by the merger. Although the methods for identification in her paper and ours are different, the interpretation of the economic channel through which the results are derived is the same. Those borrowers that are either in a strong or weak bargaining position are not affected when there is one less lender with whom to negotiate.

6.4. **Robustness.** In this subsection we analyze the robustness of our results to different sample assumptions and treatment-group definitions.

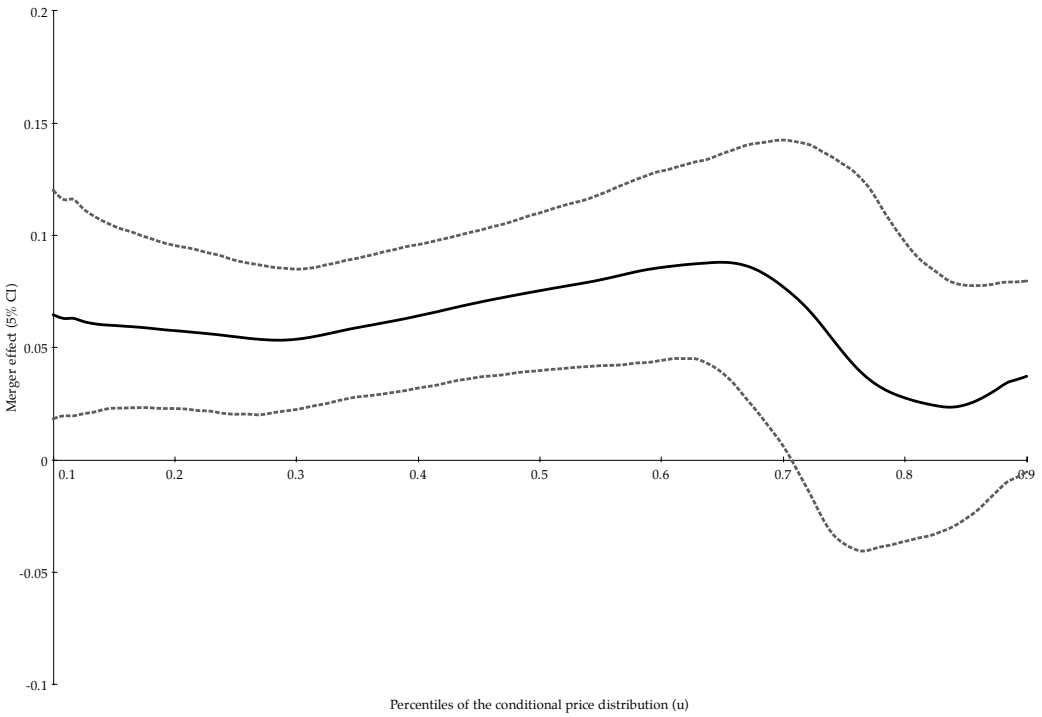


FIGURE 4. Distribution of the average treatment effect of the merger on transaction prices

First, we re-estimate the linear difference-in-difference specification assuming alternative distance rules to define the size of local markets. Recall that we assumed that consumers shop within a 5 KM radius around the centroid of their FSA's. Increasing this threshold tends to raise the number of FSA's directly affected by the merger, at the cost of including areas that are too large (falsely treated). Using a smaller threshold reduces the number of treated neighborhoods, and defines as control neighborhoods that are affected by the merger. Therefore, over-estimating or under-estimating the size of shopping areas should bias our results towards zero.

The results presented in Table 10 of the Appendix confirm this intuition. The effect of the merger is statistically different from zero for distance radii between three and six kilometers. Below and above these levels the point estimates decrease towards to zero, and we cannot reject the null hypothesis of no merger effect.

Next, we re-estimate the effect of the merger using different period windows. Throughout the paper we used contracts signed at most one year before or after the official merger date. This is a natural choice since the merged entity started closing duplicate branches about a year after the merger. Our estimates therefore capture the effect of the merger holding fixed (roughly) the distribution of branches.

In Table 8 we show that the window choice does not affect our results, as long as we use a period that does not exceed one year. In fact, the effect of the merger using a six month window is larger than the one we get using the one year window (i.e. 8 versus 6 bps). Expanding our sample to cover an eighteen-month period eliminates the merger effect. If nothing else happened during this

TABLE 8. Effect of the merger on transaction prices for alternative period windows and merger dates

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Period windows			Merger dates	
	+/- 6 Months	+/- 1 Year	+/- 1.5 Year	- 6 Months	+ 6 Months
Merger	0.0811[a]	0.0593[a]	0.0173	0.0403[c]	-0.0491[b]
	(0.0295)	(0.0202)	(0.0174)	(0.0212)	(0.0229)
Constant	7.359[a]	7.218[a]	7.187[a]	7.051[a]	7.152[a]
	(0.128)	(0.120)	(0.0784)	(0.0922)	(0.0762)
Observations	8,319	17,074	25,591	17,909	16,934
R-squared	0.054	0.059	0.065	0.054	0.070

extended period, one could infer that the merger led to a short-run price increase, and a longer run price decrease due to efficiency gains. It would be hazardous to make such a statement however, since other events occurred in this longer period, including the closure of duplicate branches, and possibly other mergers.

In columns (4) and (5) we perform a falsification exercise, in which we move the merger date by six months and use a one year window as before. Our objective is to test for the presence of an aggregate trend correlated with the merger date, that would be specific to consumers living in the treated areas, and therefore bias our results. The results of this exercise are encouraging. Moving the merger date six month earlier lowers the point estimate to 4 bps, and decreases the precision (the effect is significant at 10% level). Since the one year window includes six months after the merger period, it make sense that the coefficient would not go to zero completely.

Moreover, moving the merger date six months after changes the sign of the point estimate, which is now negative and statistically significant. This result is consistent with the estimate in column (3), obtained with the 18 months window. It appears that something happened towards the end of the one year period, and during the following year, such that interest rates went down in the treated markets relative to the control markets. If this event occurred during our main sample period, it would bias our results against finding any price increase. We are therefore confident that the documented price increase measures the change in market power caused by the merger.

7. CONCLUSION

In contrast to most of the literature studying the effects of horizontal mergers that focuses on posted prices, we take advantage of transaction level data to document important heterogeneity in the reactions of firms and consumers to a merger. Our empirical analysis exploits observed differences in the choice sets of consumers and their financial characteristics to estimate heterogenous treatment effects, and estimates the distribution of treatment effects across unobserved consumer types.

We find that the average effect of the merger under analysis is quite small, suggesting that the average consumer is able to extract a large share of the transaction surplus through search and

negotiation even when there is one fewer lending option. However, this finding masks important heterogeneity. Some borrowers pay significantly higher rates following the merger, while others are barely affected. Although in some cases the higher rates may be associated with higher market power resulting from the decrease in the number of lender options, many of those paying higher rates are contracting with the merged entity and may be benefiting from the improved level of services offered. Even more importantly, the evidence we present suggests that much of the heterogeneity in rates can be explained by differences in search costs and negotiation ability. Borrowers at the top of the price distribution, those with very high search costs/bargaining abilities, are not affected by the merger, while those lower in the price distribution are. Therefore our results imply that lender competition appears to have no impact on rates in the top part of the price distribution.

These results have important implications for the design of mortgage-market policies. If the objective is to support vulnerable borrowers (those paying the highest rates), policies designed to increase competition, or to prevent increases in concentration, such as restrictions on merger activity or prevention of bank failure may be ineffectual. Instead, what would be required would be policies designed to help borrowers search for and negotiate better terms. For instance, policies that improve the financial literacy of borrowers may help them in their negotiations. Geraldi et al. (2010) argue that financial illiteracy played an important role in the rate of foreclosures in the U.S. housing crisis.

In Canada, these borrowers could be informed as to the benefits of using brokers. Since brokers have fiduciary duties towards borrowers, their use helps borrowers search for and negotiate better terms. This is in contrast to the U.S. where, except in California, brokers do not have fiduciary duties. As a result, and as documented by Hall and Woodward (2010) there is considerable confusion surrounding the mortgage process generally, and the role of brokers in particular.[30] Although there has been some discussion about the possibility of assigning fiduciary duties to brokers in the U.S., language initially appearing in the Dodd-Frank Act that would have done so was ultimately removed.

Another potential avenue for lowering search costs is through the use of the internet. The development of the internet and other technological improvements may lower the costs of gathering information and of getting approval for particular rates. This could result in a shift in the distribution, similar to what is described in Hortaçsu and Syverson (2004). We would expect that this would increase bargaining power for all borrowers, but especially those with greater search costs, resulting in a higher degree of competition.

[30]The Wall Street Journal (Hagerty (2007)), for example, highlights the debate in the U.S. over mortgage brokers – most consumers believe that brokers represent them, however, as the president of the Colorado Mortgage Lenders Association states "The mortgage broker does not represent the borrower."

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Appendix A. Large Canadian Mergers

1993. On January 1, 1993 TD Bank acquired, under duress, Central Guaranty Trust. Even though Central Guaranty Trust had a poor balance sheet, there was substantial interest by several financial institutions in acquiring its assets from Central Capital Corporation, which owned 87 per cent of trust. At the official auction TD won over the joint bid from National Bank, Canada Trust, and Montreal Trust. Given the conditions of the Central Guaranty Trust balance sheet (they had very high risk mortgages and commercial lending activities), the Canadian Deposit Insurance Corporation provided financial support to TD in the takeover. In terms of branches, TD acquired 156 locations. TD also inherited nearly 11 billion dollars in deposits.

Soon after TD Bank's acquisition of Central Guaranty Trust, the second largest acquisition (in terms of assets) in Canada occurred, with Royal Bank acquiring Royal Trust. In 1992 Royal Trust's parent company, Royal Trustco, had experienced liquidity issues, and in early 1993 announced it was looking for a buyer of the trust company. RBC's takeover of Royal Trust was announced on March 18, 1993 and consummated on September 1, 1993. The Royal Trust brand was well-known and well-respected in the financial industry. Furthermore, most people believed that RBC was a perfect match for Royal Trust. They shared the same name, colors, and both had distinguished histories. Royal Trust had 150 branches at the time of the acquisition, largely in Ontario and Quebec, but also with a significant presence in Alberta and British Columbia. According to Competition Bureau (2003), the RBC-Royal Trust merger was analyzed by the Bureau when it was first proposed. The Competition Bureau did not place any restrictions on the merger.

1994. The Competition Bureau also did not place restrictions on Bank of Nova Scotia's (BNS) acquisition of Montreal Trust on April 12, 1994. In this instance BNS acquired 9 billion dollars in deposits and 125 branches. Montreal Trust had experienced some losses in the early 1990s because of market value deficiencies in the investments and assets, but was considered a sound financial institution and a good purchase by BNS.

1997. On August 14, 1997 Bank of Nova Scotia acquired National Trust and Victoria and Grey Mortgage Corporation, without restrictions by the Competition Bureau. In this case BNS acquired nearly 12 billion dollars in deposits and 199 branches. Most of what is known about National Trust is confidential. We do know, however, that the transaction was valued at approximately 1.25 billion dollars even though National Trust was considered a poorly run institution that had acquired an excessive number of small, failing trusts throughout the 1990s. It was largely the mis-management of the infrastructure that led National Trust to look for a buyer.

2000. The last bank merger to be approved in Canada was Toronto-Dominion Banks' (TD) acquisition of Canada Trust in 2000. The price tag was roughly 8 billion dollars (TD financed the purchase by issuing 700 million dollars in equity) and it resulted in over 400 branches being acquired as well as a strong share of the mortgage market. The merger was analyzed by the Competition Bureau and allowed to be completed under minor conditions. For example, TD had to divest in some of its branches in three of the seventy-four markets defined by the Bureau (Kitchener-Waterloo-Cambridge-Elmira, Port Hope, and Brantford-Paris). TD also had to sell CT's MasterCard credit card business (they sold the consumer credit card business to Citibank in November 2000). TD was issuing Visa credit cards at the time of the acquisition, and it was not until 2009 that Canadian banks could sell both brands simultaneously.

Appendix B. Additional tables

Table 9. Definition of Household / Mortgage Characteristics

Name	Description
FI	Type of lender
Source	Identifies how lender generated the loan (branch, online, broker, etc)
Income	Total amount of the borrower(s) salary, wages, and income from other sources
TDS	Ratio of total debt service to income
Duration	Length of the relationship between the borrower and FI
R-status	Borrowers residential status upon insurance application
FSA	Forward sortation area of the mortgaged property
Market value	Selling price or estimated market price if refinancing
Applicant type	Quartile of the borrowers risk of default
Dwelling type	10 options that define the physical structure
Close	Closing date of purchase or date of refinance
Loan amount	Dollar amount of the loan excluding the loan insurance premium
Premium	Loan insurance premium
Purpose	Purpose of the loan (purchase, port, refinance, etc.)
LTV	Loan amount divided by lending value
Price	Interest rate of the mortgage
Term	Represents the term over which the interest rate applies to the loan
Amortization	Represents the period the loan will be paid off
Interest type	Fixed or adjustable rate
$CREDIT$	Summarized application credit score (minimum borrower credit score).

Some variables were only included by one of the mortgage insurers.

TABLE 10. Effect of the merger on transaction prices for alternative competitive neighborhood assumptions

VARIABLES	(1) R=1	(2) R=2	(3) R=3	(4) R=4	(5) R=5	(6) R=6	(7) R=7	(8) R=8	(9) R=9	(10) R=10
Merger	0.0231	0.00977	0.0330[c]	0.0580[a]	0.0593[a]	0.0433[c]	0.0290	0.0252	0.0262	0.0271
	(0.0295)	(0.0170)	(0.0179)	(0.0210)	(0.0202)	(0.0226)	(0.0237)	(0.0250)	(0.0259)	(0.0266)
Constant	7.223[a]	7.432[a]	7.433[a]	7.436[a]	7.451[a]	7.477[a]	7.518[a]	7.567[a]	7.657[a]	7.704[a]
	(0.0956)	(0.0902)	(0.0744)	(0.0854)	(0.0848)	(0.0968)	(0.103)	(0.110)	(0.109)	(0.108)
Observations	7,331	18,085	20,821	18,471	17,074	15,344	13,828	12,047	10,920	10,003
R-squared	0.057	0.057	0.058	0.062	0.059	0.059	0.056	0.053	0.055	0.055
% Treated	0.411	0.537	0.605	0.633	0.618	0.631	0.610	0.610	0.587	0.561

Heteroscedasticity-robust standard-errors are in parenthesis. Significance levels: [a] p-value<0.01, [b] p-value < 0.05, [c] p-value < 0.1. The dependent variable is measured as the transaction rate minus the weekly average across all markets. The sample includes all contracts with non-missing characteristics one year before and after the merger. Neighborhoods with fewer lenders than the 1% percentile in the treatment group and more than 99% percentile in the control group are excluded (i.e. less than 5 or more than 8). Each column corresponds to a different competitive neighborhood assumption measured in Euclidian distance. The control variables include: income, loan-size, loan/income interaction, loan-to-value categories (3), credit score category (5), residential status (i.e. parents or renter), bank, month and province dummies.